AD-A285 853

PL-TR-94-2207

(1)

A FINAL REPORT ON THE MARUSSI HYPOTHESIS IN DIFFERENTIAL GEODESY

J. D. Zund

New Mexico State University
Department of Mathematical Sciences
Las Cruces, NM 88003-0001

13 June 1994

Scientific Report No. 7

94-30324

PHILLIPS LABORATORY
Directorate of Geophysics
AIR FORCE MATERIEL COMMAND
HANSCOM AIR FORCE BASE, MA 01731-3010

94

"This technical report has been reviewed and is approved for publication"

(Signature)
DAVE GLEASON
Contract Manager

(Signature)
FRANK MARCOS, Acting
Branch Chief

(Signature)
WILLIAM K. VICKERY
Division Director

This report has been reviewed by the ESC Public Affairs Office (PA) and is releasable to the National Technical Information Service (NTIS).

Qualified requestors may obtain additional copies from the Defense Technical Information Center (DTIC). All others should apply to the National Technical Information Service (NTIS).

If your address has changed, if you you wish to be removed from the mailing list, or if the addressee is no longer employed by your organization, please notify PL/TSI, 29 Randolph Road, Hanscom AFB, MA 01731-3010. This will assist us in maintaining a current mailing list.

Do not return copies of this report unless contractual obligations or notices on a specific document requires that it be returned.

REPORT DOCUMENTATION PAGE

Form Approved
OMB No. 0704-0188

1. AGENCY USE ONLY (Leave blank)

2. REPORT DATE
13 June 1994

3. REPORT TYPE AND DATES COVERED
Scientific Report No. 7

4. TITLE AND SUBTITLE
A Final Report on the Marussi Hypothesis in Differential Geodesy

5. FUNDING NUMBERS
PE 61102 F
PR 2309 TA G1 WU BZ
Contract
F 19628-89-K-0044

6. AUTHOR(S)
J. D. Zund

7. PERFORMING ORGANIZATION NAME(S) AND ADDRESS(ES)
New Mexico State University
Department of Mathematical Sciences
Las Cruces, NM 88003-0001

8. PERFORMING ORGANIZATION REPORT NUMBER

9. SPONSORING/MONITORING AGENCY NAME(S) AND ADDRESS(ES)
Phillips Laboratory
29 Randolph Road
Hanscom AFB, MA 01731-3010
Contract Manager: David Gleason/GPIM

10. SPONSORING/MONITORING AGENCY REPORT NUMBER
PL-TR-94-2207

11. SUPPLEMENTARY NOTES

12a. DISTRIBUTION/AVAILABILITY STATEMENT
Approved for public release;
Distribution unlimited

12b. DISTRIBUTION CODE

13. ABSTRACT (Maximum 200 words)

This report contains a detailed discussion of the primary conceptual aspects of the Marussi-Hotine theory of differential geodesy, and how it stands today. In addition to reviewing the Marussi hypothesis and Ansatz, we present a derivation of why Hotine's normal coordinate system is admissible, and a new conjecture on the type of local coordinate systems permitted in the Marussi-Hotine theory.

14. SUBJECT TERMS
Marussi hypothesis, Marussi Ansatz, Hotine Problem, (ω, ϕ, N) coordinate system, the Zund conjecture.

15. NUMBER OF PAGES
28

16. PRICE CODE

17. SECURITY CLASSIFICATION OF REPORT	18. SECURITY CLASSIFICATION OF THIS PAGE	19. SECURITY CLASSIFICATION OF ABSTRACT	20. LIMITATION OF ABSTRACT
Unclassified	Unclassified	Unclassified	SAR

CONTENTS

"A FINAL REPORT ON THE MARUSSI HYPOTHESIS IN DIFFERENTIAL GEODESY"

J.D. Zund

Summary:

The present report was originally written as an invited paper to be presented at the III Hotine-Marussi Symposium on Mathematical Geodesy (L'Aquila, May/June 1994). As circumstances developed I was not able to attend this conference and only a very brief version of the report will be submitted for publication. The manuscript is rather detailed and gives a full discussion of the primary conceptual aspects of the Marussi-Hotine theory of differential geodesy, and how it stands today. In addition to reviewing the theory, the report includes a derivation of why Hotine's normal coordinate system is inadmissible, and a conjecture on the type of local coordinate systems which are permitted in the Marussi-Hotine theory.

1 INTRODUCTION

The purpose of this paper is to give a final report on my research concerning the Marussi-Hypothesis and the corresponding Marussi-Hotine, (**MH**), theory of differential geodesy. I will discuss not only its achievements, but also its limitations and the prospects that it offers for future development.

Since my attention will primarily be focussed on the latter two aspects of the **MH**-theory, let me briefly comment on the achievements and significance the **MH**-approach for theoretical geodesy. First, I would say that, regardless of the success, or failure, of the **MH**-theory, it has been important in that it has conclusively demonstrated the possibilities and power of the use of differential-geometric techniques in geodesy. Moreover, it is largely due to Marussi and Hotine that geodesists have become acquainted with tensors and differential forms. Truly, as Sir Alan Cook wrote in his preface to Marussi (1985), Marussi

> "... left us thinking about the gravity field of the Earth and of geodesy in ways very different from those he found."

Likewise, Hotine by the sheer force of his personality and intellect, as indicated in his monumental treatise, Hotine (1969); and collected differential geodetic papers, Hotine (1992); has left a lasting impression on our thinking. While I will argue in this report that the **MH**-theory is limited in its applicability, the methodology which they pioneered in their original expositions are noteworthy and may ultimately turn out to be of lasting importance.

The fact that creators of a new scientific theory often fail to formulate it in final form, or grasp the ultimate significance of their work, is familiar to historians of science. I am inclined to liken the efforts of Marussi and Hotine to those of Sir William Rowan Hamilton [1805-1865] and his mathematical work on quaternions. Hamilton was firmly convinced that quaternions represented his greatest achievement -- indeed, he rated their discovery as of as great an importance for the middle of the 19th century as the discovery of the calculus was for the close of the 17th century! -- yet today they are largely ignored. However, the effort which he lavished on their development during the last twenty years of his life was of vital importance for the mathematical and physical sciences. As a legacy, they have left us a rigorous theory of complex numbers, a new conception of vectors (which resulted in vector analysis as a byproduct), and gave impetus to a serious investigation of the general structure of algebras. Relative to this last topic, quaternions provided a concrete example of a non-commutative algebra, or more precisely a division algebra (or ring), which l. led to a rich mathematical theory which includes as a special case, matrix theo Of course, without Hamilton, these things would undoubtably have been done, bu his enthusiasm and influence probably hastened their development. Like Hamilton, Marussi and Hotine may not have done what they sought to do, but nevertheless their work is undeniably the first step in the geometrization of geodesy. While it is a fool's game to prognosticate about the future, in Section 5 I will suggest an extension of

the **MH**-theory, the *generalized* **MH**-*theory*, which appears to offer some interesting possibilities.

Before proceeding with a detailed critique of the **MH**-theory, it should be noted that the theory in their hands was somewhat loosely definited. It was formally initiated in Marussi (1949) and was actively pursued by him during the years 1947-1960, while Hotine's contributions appeared in 1957-1968 and culminated in his posthumous treatise, Hotine (1969). As these dates indicate, Marussi's main period of geodetic research effectively ended about the time that Hotine's activities began. While it is clear that Marussi greatly influenced Hotine — see his acknowledgement on pages xiii-xiv of Hotine (1969) — Hotine's influence on Marussi is less obvious. Indeed, while one could think of Marussi as the architect and Hotine as the constructor of the theory (see Zund (1990a), for a discussion of their backgrounds) it is not evident that Marussi was in complete accord — or even satisfied — with the results of Hotine's efforts. In effect, although they were close friends, they both had strong personalities, held independent views, and employed almost totally different methodologies. As such, their work may be regarded as complementary with the caveat that to a surprising extent there was little overlap in their research on specific topics, viz. Marussi did one thing while Hotine did another. Hence, in reality the **MH**-theory is an abstraction and superposition of their efforts and gaps occur. While one can extract and lay out a common mathematical framework for their work, their emphasis, feelings, and hopes are frequently unstated in their writings. However, since the broad context of their theory is well-defined, i.e. it is the physical differential geometry of the gravity field in 3-dimensional Euclidean space, the omissions in their expositions do not pose a serious hindrance to an understanding of what their theory must be about. Nevertheless on more than one occasion, in trying to unravel a knotty issue, I have fervently wished that it would be possible to ask Marussi or Hotine — preferrably both — what they had in mind on a particular topic (see Zund (1994) for a discussion of unsolved puzzles in the **MH**-theory).

2 AN OVERVIEW OF MH-THEORY

Marussi came to the basic ideas of his theory, *geodesia intrinseca*, after fifteen years of practical experience of surveying obtained as a geographical engineer at the Istituto Geografico Militare. Until the appearance of Marussi (1947), which contained his first sketchy and tentative account of intrinsic geodesy, essentially he had published no theoretical work in geodesy. However, the years 1932-1947 were not idle, but constituted a period of profound gestation during which, armed with a doctorate in pure mathematics (Università di Bologna 1932), he embarked in a voracious digestion of the geodetic literature. His reading included not only the geodetic classics of Helmert, Jordan and Eggert, etc., but also led him into the more esoteric geometric and physical monographs of Cartan and Weyl (see Zund (1990a) for specific citations). Hence, Marussi (1949), was not an ordinary paper on geodesy, but rather the product of deep prolonged reflection and thought about how a physical theory should be formulated. It's profundity was apparently lost on most of his listeners. Indeed, it

was presented in Section V: The Geoid under the presidency of J. de Graaf Hunter, and the secretary, G. Bomford (1949) wrote the following dispassionate summary of it:

> "This paper gives an account of the possible application of the methods of vector analysis to the study of the Earth's gravitational field and of the simplifications which may be derived from its use. It concludes that the most useful programme of work would be to make such observations of gravity, the deviations of the vertical, and their gradients, as can best determine the second derivatives of the potential of intervals of (say) 10 minutes of latitude and longitude over a large area."

But Hotine recognized its importance — and the spark of genius — in his ideas (see Zund (1991)). The 1949 paper was followed by a literal flurry of publications of which Marussi (1951) was the most detailed exposition of his ideas. His 1951/52 American lectures, which I had the honor and privilege of revising and republishing as Marussi (1988), are of particular interest, since they were given at the peak of his geodetic activity, and knowing that his audience would not be familiar with his motivation and viewpoint, they were unusually detailed and included material assumed known in his formal publications. Marussi's theory can best be comprehended by regarding it as set out in three stages:

1^0 fundamental ideas;

2^0 basic rules/conditions for investigation; and

3^0 method for realizing in practice the theory given in 1^0 and 2^0.

The first stage considered of the observation that a physical theory can be formulated in two ways: in an *absolute* manner for which the fundamental ideas and basic equations are given independent, or without preference, of the choice of a particular reference system; and a *relative* manner in which special reference systems are chosen and utilized. The *absolutist* and *relativist* formulations are reminiscent of those in Euclidean geometry in which one has two versions of the theory — a synthetic and an analytic version. The former gives the basic assumptions and notions in a pristine logical form (which is primarily concerned with the various interrelationship occurring in the structure) while the latter is concerned with a useful analytic formulation of this structure.

The second stage involves setting out certain guidelines, or working rules, for carrying out an investigation of the theory. These include the following eight assumptions:

(i) the geometry of space is Euclidean and three-dimensional;

(ii) the equipotential surfaces of the Earth's gravity field are locally isometrically imbedded in a three-dimensional Euclidean space;

(iii) the choice of reference systems should be natural and not contrived;

(iv) the reference systems should involve no additional hypotheses or otherwise impose any loss of generality in our description of the gravity field;

(v) the reference systems employed in the analysis must be susceptible of an immediate physical interpretation;

(vi) the components of all vectors/tensors occurring in the theory should be readily measurable;

(vii) the domain of the reference systems must be sufficiently large to be useful, viz. to allow one to make measurements and give a description of the gravity field in a required vicinity;

(viii) the domains of various reference systems should be continuable, or extendable, in the sense that they are compatible and one can readily pass between neighboring systems of reference.

Actually Marussi (1988) omitted **(i)** and **(ii)** which he assumed as obvious in order that the conceptual framework of classical Gaussian differential geometry be employed. While each of the *Marussi Conditions* **(i)-(viii)** is reasonable, they are remarkable not only for their comprehensive character, but also for the rigid standard they require of the theory. Seldom in the history of science, has a physical theory been proposed — virtually at its inception — with such lucidity and precision.

The third stage then seeks a methodology in which the previous two stages, and as many of the guidelines **(i)-(viii)** as possible, can be realized. Marussi believed that this could be achieved by using a family of privileged coordinate systems which he called *intrinsic coordinates.* This is the *Marussi Hypothesis*, and originally he stated his conditions in terms of (local) coordinate systems and not reference systems as we have done. The adjective 'intrinsic' apparently stems from his conviction that such 'privileged coordinates' exist and can be found. In a sense the word 'intrinsic' is overused in mathematics, particularly in geometry, where usually it conveys more feeling than actual content. Often it is literally employed as a synonym for 'very nice', or 'natural', with the implied suggestion that other choices are 'less nice', over even 'unnatural'. For example, in Newtonian dynamics inertial systems would be intrinsic, while non-inertial systems would not be intrinsic. With conditions **(iii)-(vi)** in mind, Marussi then took the bold step of making the *Ansatz* $\mathfrak{A}$ that curvilinear coordinates x^r could be chosen so that

$$\mathfrak{A}_1 : x^3 \equiv N,$$

where N is the geopotential function of the gravity field, and

$$\mathfrak{A}_2 : x^\alpha \equiv u^\alpha, \qquad (\alpha = 1, 2)$$

where the u^α are the Gaussian parameters on an equipotential surface

$$\mathrm{S} : N = \text{constant}.$$

Symbolically we write $(\mathfrak{A}) = (\mathfrak{A}_1) + (\mathfrak{A}_2)$, and note that these properties essentially characterize the **MH**-theory of differential geodesy. The assertions are independent, and neither is immediately obvious. However, it is undeniable that if they could be achieved they would apparently yield a great simplification in the mathematical structure of the theory. It is not clear whether either of these choices had previously been employed in theoretical geodesy, prior to Marussi, although it is hard to believe that someone had not entertained such possibilities. In any case, unquestionably Marussi and Hotine were the first investigators to systematically base a geodetic theory on such a *Kunstgriff* (artifice) as $\mathfrak{A}$.

The immediate question is whether $\mathfrak{A}$ is legitimate, i.e. whether it is compatible with **(iii)** and **(iv)**. During the lifetimes of Hotine and Marussi, no one was able to sort out this issue, and in the absence of conclusive evidence against $\mathfrak{A}$, they did the obvious thing: i.e. they employed it and sought to investigate its consequences. One of these was the *Hotine Problem* which proposes constructing a family of geodetically useful coordinate systems satisfying $\mathfrak{A}$. His solution, which we will discuss in Section 3, is called the *Hotine Hierarchy* $\mathfrak{H}$ and consists of five purported valid coordinate systems $\mathfrak{H}_0, \cdots, \mathfrak{H}_4$, which were derived in Part II of Hotine (1969). As an example of the dichotomy of methodologies mentioned at the end of Section 1, we note that in his work Hotine made *no explicit mention* of conditions **(i)-(viii)**, and it is not known whether he knew of them[1], or would have regarded them as either agreeable/satisfactory.

Relative to the acceptance of the *Marussi Ansatz*, three alternatives immediately come to mind:

1) a choice such as $(\mathfrak{A})$ is certainly mathematically admissible, since *a priori* the tensor calculus permits the use of any (local) coordinate systems, say x^r and $\overline{x}^r$, as long as they are *regular* (i.e. smoothly differentiable of some order, and invertible);

2) maintains that while the alternative **1)** is true, the choice of $(\mathfrak{A})$ is not just *any* coordinate system, but one which is inextricably tied to a particular geometric situation, i.e. a family of equipotential surfaces (*N-surfaces*) and their plumblines (*N-lines* = orthogonal trajectories of the N-surfaces). One then inquires whether this situation necessarily demands use of $\mathfrak{A}$, or whether $(\mathfrak{A})$ actually imposes conditions generally consistent with the geometric situation?;

3) observes that $(\mathfrak{A})$ is not useful in the general working framework of theoretical geodesy, i.e. in *determining* the geopotential function for a given geodetic situation.

Presumably, Marussi and Hotine consciously, or otherwise, took the alternative **1)** and left it to adherents of **2)** to produce evidence of the restrictiveness of $(\mathfrak{A})$. Hence

[1] The original manuscript of Marussi (1988) was dated 12 March 1952 and was Technical Paper No. 159 of the Mapping and Charting Research Laboratory of the Ohio State University Research Foundation. It was apparently not widely distributed, and I obtained my copy from Prof. E.W. Grafarend.

2), until recently remained a *possibility* with only insufficient evidence to confirm or deny it. Probably, alternative **3)** was the common view of most people who did not seriously study the **MH**-theory. Actually, as we will further discuss in Section 5, **3)** is a substantive contention, although the reason for it being so were likely not properly understood. The fact is that — without becoming deeply involved in the mathematical details — $(\mathfrak{A})$ implicitly requires that the geopotential function N be *known*. Hence, $(\mathfrak{A})$ reduces the **MH**-theory to being a *descriptive theory* in which one is concerned exclusively with *describing* a given gravity field, rather than *determining* an *unknown* N subject to specified geodetic/geometric circumstances. Of course, neither Marussi nor Hotine could have failed to notice this fact, but curiously it is *nowhere emphasized* in their writings.

Finally, it is reasonable to inquire which part of $(\mathfrak{A})$, i.e.$(\mathfrak{A}_1)$ or $(\mathfrak{A}_2)$, is the most restrictive and, hence, most likely to cause difficulties. The obvious answer is that $(\mathfrak{A}_1)$ is the stronger condition, and that if it is imposed, then $(\mathfrak{A}_2)$ can be obtained almost effortlessly by a coordinate transformation. This may be true, however, it does not insure that the choice $(\mathfrak{A}_2)$ does not somehow incur a loss of generality as suggested in alternative **2)**. While my results presented in the next section show why $(\mathfrak{A})$ is mathematically restrictive, it does not suffice to identify which of $(\mathfrak{A}_1)$ and $(\mathfrak{A}_2)$ plays the conclusive rôle in $(\mathfrak{A})$. It would appear that $(\mathfrak{A}_2)$ is the weaker condition, but in this regard it should be remembered that $(\mathfrak{A}_2)$, in effect, is a form of the Mongian parametrization of surfaces. This is important since it is Gaussian, rather than Mongian, differential geometry which gives a *complete description* of the classical differential geometry of surfaces in three-dimensional Euclidean spaces.

3 THE HOTINE PROBLEM AND ITS SOLVABILITY

Before beginning our discussion of the *Hotine Problem* (**HP**), and his solution of it, i.e. the *Hotine Hierarchy* $\mathfrak{H}$, it is necessary to step back and consider why these things are of crucial significance for the **MH**-theory of differential geodesy. Indeed, we will argue that the **HP** is the most important problem in their theory, and that in effect, the ultimate usefulness of the **MH**-theory depends on the solvability of this problem. In this section we will present a critique of the **HP** and $\mathfrak{H}$, and make a conjecture which we believe essentially categorizes the **MH**-theory and its applicability.

In a sense the **HP** really concerns how, and to what extent, one can bridge the gap between the absolutist and relativist approaches to the theory. In this respect, since the basic mathematical tool employed in the discussion is the tensor calculus, one must have a clear understanding of the principal advantages and difficulties which are encountered when any geometric/physical theory is formulated in tensor-theoretic terms. While there is no question that the creation of the tensor calculus by Ricci and his coworkers was one of the major mathematical achievements of importance to physics during the nineteenth century, the mere fact that is employed in a specific context is no guarantee that the resulting theory is either correct, or of any practical

use. A prime example of such a situation is furnished by Einstein's work on relativity. His original theory, special relativity (which made no use of tensors), was set out in 1905 and during the next decade achieved widespread acceptance and became one of the seminal parts of modern physics. In contrast, his relativistic theory of gravitation, general relativity, (which made essential use of the tensor calculus and is virtually incomprehensible without tensors), followed in 1915, but did not become of immediate relevance for almost a half century until it became important in relativistic astrophysics in the theory of gravitational collapse of neutron stars, e.g. black holes. While many factors — both mathematical and physical — contributed to this delay, one aspect is easily understood: the inherent complexity and difficulty of solving the Einstein equations in a situation which is of direct physical significance. The core of these difficulties was the transition from a rather obvious coordinate system (a spherically symmetric non-rotating coordinate system, the Schwarzschild solution of 1915/16 for what we now interpret as a non-rotating black hole) to a vastly more complicated coordinate system describing rotating black holes as in the celebrated Kerr solutions of 1963/65. Without this new coordinate system, and the special mathematical apparatus which Kerr devised to work with it, this new physically relevant solution of Einstein's equations would not have been possible. The new results involved no change in the Einstein equations, or their structure, but really consisted in the construction of a convenient coordinate/reference system.

Since quaternions have been mentioned in Section 1, we mention another example in which for our present purposes the reader may mentally replace the term 'quaternion' by 'tensor'. At the end of the last century a major controversy raged over the relative merits and superiority of the quaternion and vector formalisms. Finally, the great British mathematician, Arthur Cayley [1821-1895] was drawn into the fracas, and in 1894, in one of his last papers, he wrote:

> "My own view is that quaternions are merely a particular method, or say a theory, in coordinates. I have the highest admiration for the notion of a quaternion; but ... as I consider the full moon far more beautiful than any moonlit view, so I regard the notion of a quaternion as far more beautiful than any of its applications. As another illustration ... I compare a quaternion formula to a pocket-map — a capital thing to put in one's pocket, but which for use must be unfolded: the formula to be understood, must be translated into coordinates."

As noted in Zund (1991), the notion of a quaternion is not totally irrelevant to the **MH**-theory, since it was the progenitor of the homographic calculus which was Marussi's favorite approach to the tensor calculus.

Hence, in order to be more than a particular methodology, the tensor calculus requires having at its disposal various local coordinate systems — hopefully more than one! — which permit it to be applied, or to use Cayley's term, *unfolded*, in order to be applicable in a variety of different geometric/physical situations. The **HP** is specifically concerned with constructing such systems — always subject to $(\mathfrak{A})$ — and $\mathfrak{H}$ is the solution which he proposed in Part II of his treatise, Hotine (1969).

The Hotine hierarchy $\mathfrak{H}$ thus consists of the following five (local) coordinate systems:

$\mathfrak{H}_0$: (ω, ϕ, N)-system of Chapter 12;

$\mathfrak{H}_1$: normal-system of Chapter 15;

$\mathfrak{H}_2$: triply-orthogonal-system of Chapter 16;

$\mathfrak{H}_3$: (ω, ϕ, h)-system of Chapter 17;

$\mathfrak{H}_4$: symmetric (ω, ϕ, h)-system of Chapter 18,

(the references are to chapters of Hotine (1969) and the Gaussian parameters $u^\alpha = (\omega, \phi)$ are respectively the longitude and latitude on the N-surface $\mathbf{S}$). The construction of $\mathfrak{H}_0$ marked Hotine's first major contribution, Hotine (1957a, 1957b), to differential geodesy. The system $\mathfrak{H}_0$ was introduced by Marussi (1947, 1949), and, in effect, Hotine (1957a) represented Hotine's attempt to recase Marussi's work on a purely tensor theoretic basis without reference to the ideas of the homographic calculus. In this paper, the metric tensor associated with $\mathfrak{H}_0$ was called the 'Marussi metric', however, probably at Marussi request, this terminology was not employed in Hotine (1969). The content of Hotine (1957b) was devoted to seeking more general coordinate systems, however, the results are tentative and should be regarded — at best — as forerunners of the systems $\mathfrak{H}_1$, $\mathfrak{H}_3$ and $\mathfrak{H}_4$. None of these are fully worked out in Hotine (1957b), and it is now merely of historical interest in that it shows that, *ab initio*, Hotine was trying to solve the **HP**. It is interesting to observe, that although he occasionally toyed with the idea of systems more general than $\mathfrak{H}_0$, Marussi never seriously became involved with solving the **HP**. However, (see our discussion of $\mathfrak{H}_3$ below), there is evidence that Marussi keenly followed his friend's endeavors to construct new coordinate systems.

The hierarchy $\mathfrak{H}$ has several curious features. First, it is by no means clear how Hotine thought of it, and whether he really regarded it — as a whole — as a *bona fide* solution of the **HP**. In his preface to Hotine (1969), (see page xi), he wrote:

> "Part II deals with coordinate systems of special interest in geodesy. In Chapter 12, the properties of a general class of three-dimensional systems are developed from a single-valued, continuous and differentiable scalar N which serves as one coordinate, while the other two coordinates are defined by the direction of the gradient of N. In Chapters 15-18, the scalar N is restricted to provide simpler systems, whose properties can then be derived at once from the general results of Chapter 12."

A literal interpretation would then suggest that Hotine regarded each of the $\mathfrak{H}_i$ $(i = 1, 2, 3, 4)$ as specializations of $\mathfrak{H}_0$, viz.

$$\mathfrak{H}_i \subset \mathfrak{H}_0 \qquad (i = 1, 2, 3, 4)\,, \tag{1}$$

where "$\subset$" indicates inclusion of $\mathfrak{H}_i$ as a subcase of $\mathfrak{H}_0$. An examination of these $\mathfrak{H}_i$ shows that such a suggestion is false, although it is true that

$$\mathfrak{H}_3 \subset \mathfrak{H}_1 \tag{2}$$

and

$$\mathfrak{H}_4 \subset \mathfrak{H}_2. \tag{3}$$

The second feature is concerned with how general did he regard $\mathfrak{H}_0$. On page 69, he begins his discussion of $\mathfrak{H}_0$ by referring to it as

> "... a special, but quite general, coordinate system ...,"

so his feelings are rather ambiguous. What is clear is that he devoted more space to $\mathfrak{H}_0$ (almost twenty pages) than the other $\mathfrak{H}_i$ $(i = 1, 2, \cdots, 4)$. However, a mere page count is somewhat misleading since Chapter 12 contains much more, e.g. his theory of the Marussi tensor, than merely an explication of the properties of $\mathfrak{H}_0$ *per se*. My personal suspicion, not withstanding his remarks quoted above from the preface, is that Hotine considered $\mathfrak{H}_0$ as a *prima-facie* example of what could, and ultimately should, be done with the remaining $\mathfrak{H}_i$ $(i = 1, 2, 3, 4)$. If this were the case — and we believe it was the most likely possibility — then, as we will see, Hotine was sadly mistaken.

Since it turns out $\mathfrak{H}_0$ is the *only system* in $\mathfrak{H}$ which is valid, we will defer our discussion of it to Section 4 where its rôle in the **MH**-theory will be explained. Our immediate task is then to indicate why the systems $\mathfrak{H}_i$ $(i = 1, 2, 3, 4)$ are defective. By virtue of (2) and (3), we need consider only two systems $\mathfrak{H}_1$ and $\mathfrak{H}_2$ since their failure automatically exclude $\mathfrak{H}_3$ and $\mathfrak{H}_4$ respectively.

We first consider the normal systems $\mathfrak{H}_1$ and $\mathfrak{H}_3$. The notion of such a system is at first glance one of the most attractive possibilities in differential geodesy and it is truly disappointing that such a system is impossible to realize in practice. The idea for $\mathfrak{H}_1$ is derived from Riemannian geometry and the notions of Riemannian, geodesic, or normal coordinates although the terminology in the literature is not quite uniform (see Weatherburn (1938) or Synge and Schild (1949) for a discussion). The general theory is valid for an n-dimensional Riemannian space $\mathbf{V}_n$, however, for our purposes we will always take $n = 3$. Essentially, the idea is very simple: starting from a surface — which one calls a *base surface* and denotes by $\mathbf{S}^0$ — one considers a family of orthogonal trajectories of S^0, say $\mathbf{\Gamma}$, which by hypothesis are geodesics of $\mathbf{V}_3$. In geometric language, the orthogonal trajectories $\mathbf{\Gamma}$ of $\mathbf{S}^0$ are said to be a *normal geodesic congruence*, and one then considers the corresponding family of surfaces $\mathbf{\Sigma} = \{\mathrm{S}^1, \mathrm{S}^2, \cdots\}$ which also have the original congruence $\mathbf{\Gamma}$ as their orthogonal trajectories. This configuration is known to exist in $\mathbf{V}_3$, although its construction is not altogether trivial. The members of $\mathbf{\Sigma}$ are parallel to each other and also to $\mathbf{S}^0$, and the resulting system of surfaces is said to be a *geodesic parallel system*, or simply a *parallel system* $\mathcal{P}$. The line element for $\mathcal{P}$ is given by

$$ds^2 = g_{\alpha\beta} dx^\alpha dx^\beta + g_{33} \left(dx^3\right)^2 \tag{4}$$

where $(\alpha, \beta = 1, 2)$, and by virtue of $(\mathfrak{A}_1)$ and $(\mathfrak{A}_2)$ this reduces to

$$ds^2 = a_{\alpha\beta} du^\alpha du^\beta + \frac{1}{n^2} dN^2 \tag{5}$$

where $a_{\alpha\beta}$ is the first basic tensor, i.e. the coefficients of the first basic form **I** of **S**, and n is the *local gravity*, viz.

$$n^2 = g^{rs} N_r N_s \tag{6}$$

where N_r is the gradient of the geopotential function N. Equation (4) is Hotine's line element for his $\mathfrak{H}_1$ system. Starting from (5) Hotine ingeniously derives the properties of $\mathfrak{H}_1$ which include a set of equations — the ***variational equations*** — which purportedly describe how the first, second, and third basic tensors $a_{\alpha\beta}$, $b_{\alpha\beta}$ and $c_{\alpha\beta}$ respectively vary from surface to surface of $\mathbf{\Sigma}$ as one moves along $\mathbf{\Gamma}$ in space. His variational equations are — in a notation slightly different from that employed by Hotine — as follows:

$$\begin{aligned} \frac{\partial a_{\alpha\beta}}{\partial \ell} &= -2 b_{\alpha\beta}, \\ \frac{\partial b_{\alpha\beta}}{\partial \ell} &= \varphi_{\alpha\beta} - c_{\alpha\beta}, \\ \frac{\partial c_{\alpha\beta}}{\partial \ell} &= \psi_{\alpha\beta}; \end{aligned} \tag{7}$$

where ℓ is the arclength along $\mathbf{\Gamma}$ and

$$\begin{aligned} \varphi_{\alpha\beta} &:= n \left(n^{-1}\right)_{\alpha\beta}, \\ \psi_{\alpha\beta} &:= a^{\rho\sigma} \left(b_{\alpha\rho}\varphi_{\beta\sigma} + b_{\beta\rho}\varphi_{\alpha\sigma}\right); \end{aligned} \tag{8}$$

with the covariant differentiation in the set (8) being taken with respect to the surface metric. The variational equations (7) are original and do not occur in Gaussian differential geometry, except in the simple case when

$$\varphi_{\alpha\beta} = \psi_{\alpha\beta} = 0 \tag{9}$$

which corresponds to $\mathbf{\Gamma}$ reducing to a linear convergence $\mathbf{\Lambda}$ of straight lines. Hotine's derivation of them is not totally convincing (although it is not quite clear that it is wrong!). The difficulty of assessing the validity of $\mathfrak{H}_1$ is that (7) is very attractive, despite the fact that one cannot either integrate them or show that they are inconsistent. I have spent a great deal of time trying to work with them, and ultimately came to the conclusion that relative to the status of $\mathfrak{H}_1$ they are a red herring. The equations *per se* are formally consistent and as such cannot be used to show that (9) is the *only case* which occurs within the context of Gaussian differential geometry. Such a result is indeed the case, but its derivation comes from other aspects of the construction of $\mathfrak{H}_1$ and not from (7).

The defect in the construction of $\mathfrak{H}_1$ consists in the fact in addition to the assumptions $(\mathfrak{A}_1)$ and $(\mathfrak{A}_2)$ of Section 2 the line element (5) demands that

$$(\mathfrak{A}_3) \quad g_{\alpha 3} = 0$$

which is equivalent to $g^{\alpha 3} = 0$. Hence, upon writing $u^\alpha = (U, V)$, Hotine's requirement of the orthogonality of the gradients of U, V and N [his unnumbered equations on page 104], reduces to

$$\begin{aligned} g^{rs} U_r N_s &= 0, \\ g^{rs} V_r N_s &= 0. \end{aligned} \tag{10}$$

When these conditions are combined with the Hotine 3-leg vectors it follows that these gradients assume the form (where a, b, h, and k are scalars)

$$\begin{aligned} U_r &= a\lambda_r + h\mu_r, \\ V_r &= k\lambda_r + b\mu_r, \\ N_r &= n\nu_r, \end{aligned} \tag{11}$$

and $D := ab - hk \neq 0$. But then the Pfaffian forms associated with (5) become

$$\begin{aligned} \theta_1 &= (bdU - hdV)/D \\ \theta_2 &= (-kdU - adV)/D \\ \theta_3 &= dN/n. \end{aligned} \tag{12}$$

When these are substituted into the Cartan structural equations one finds that

$$a_{/3} = b_{/3} = h_{/3} = k_{/3} = 0 \tag{13}$$

where "/" denotes the directional derivative in the $\boldsymbol{\nu}$ direction which is tangent to $\mathbf{\Gamma}$ and, hence, orthogonal to $\mathbf{S}$. The conditions (13) are highly restrictive and require that both the Gaussian and Germain curvatures of $\mathbf{S}$ vanish identically[2]. The vanishing of *either* of these surface curvatures is inadmissible and shows that the construction is invalid.

Hence, what has actually been shown is that the usual conditons $(\mathfrak{A}_1)$ and $(\mathfrak{A}_2)$, i.e. the full Marussi Ansatz, is inconsistent with the *Hotine Ansatz* $(\mathfrak{A}_3)$ when $\mathbf{\Sigma}$ is a non-trivial family of curved surfaces. The only admissible case turns out to be when n = constant, i.e. the local gravity is constant, and this not only trivializes the line element (5) but also shows why (9) is the only possible case in Gaussian differential geometry.

The triply-orthogonal system $\mathfrak{H}_2$ and its special subcase $\mathfrak{H}_4$ is a curious situation. The presentation of the former in Chapter 16 of Hotine (1969) is very brief (less than three pages) and barely an outline, so for details of his reasoning one must turn to Hotine (1966a, 1966b). Apparently, it was a favorite topic of his, and the first mention of it occurs in Hotine (1965). The issue centers around the Cayley-Darboux equation,

[2]In terms of the leg coefficients discussed in Section 4 and exhibited in (**), (13) requires that $k_1 = k_2 = t_1 = \varepsilon_3 = 0$.

which is a very complicated non linear third order partial differential equation which the geopotential function must satisfy. We write this equation symbolically as

$$\aleph N = 0, \tag{14}$$

and in Zund and Moore (1987a) it was noted that in its most general form (14) involves 324 terms! Hotine believed that any smooth function could be chosen as a member of a triply-orthogonal family of surfaces $\mathcal{O}_3$ in a Euclidean 3-space, (this is known as the *Hotine Conjecture*), which is tantamount to the assertion that (14) is *always identically satisfied*! The falsity of this conjecture was conclusively settled in Zund and Moore (1987b), however, it is almost inexplicable how Hotine could have held to his views in spite of the existing evidence to the contrary. In Hotine (1964), (14) was acknowledged to be "a complicated partial-differential equation" and sometime later, Hotine derived an erroneous version of it — see equation (16.04) on page 114 of Hotine (1969) — which he apparently refused to believe was not genuine. The root of the difficulty was that his version of (14) was an identity, in fact, one of the Bianchi identities which are, of course, always identically satisfied in a 3-dimensional Euclidean space. The evidence against his conjecture, viz. that (14) is not an identity, was overpowering yet Hotine would not accept it. For example, at the beginning of Chapter XI of Forsyth (1920) — one of Hotine's favorite references and probably where he first learned differential geometry — it is explicitly stated:

> ".. in 1846, it was pointed out by Bouquet that any arbitrarily chosen surface cannot belong to a triply-orthogonal system. In 1862 Bonnet had shown that the determination of such a system must depend upon a partial differential equation of the third order; and this equation of the third order was first obtained by Cayley in 1872. Soon there followed the researches of Darboux ... and many other workers, among whom Bianchi may be specially named, have laboured in the field."

Needless to say, (see the multitude of references in Zund and Moore (1987)), geometers of the stature of Bouquet, Bonnet, Cayley, Darboux, and Bianchi did not miss much, and it is incredible that they could have failed to note that (14) was an identity, if this had been the case. Since Hotine did not challenge the procedure leading to the Cayley Darboux equation, he could only have thought that everyone — except himself! — had made a horrendous calculational error. However, this is unlikely, since merely to cite the example of Cayley, it has been said that in his 966 papers his computational errors can be counted on one hand! Moreover, Marussi was unable to dissuade Hotine from his belief in his conjecture, and in Marussi (1967) — without explicit reference to Hotine's work — he stated

> "... the family of equipotential surfaces of the Earth's gravity field is not of Lamé's type (for it to be so, Darboux's third order partial differential equation would need to be satisfied) ..."

This was not an idle comment on Marussi's part, he had had a doctoral student investigate the situation, see the unpublished *Tesi di Laurea* of N. Milani (1960).[3] This thesis, essentially dealt with various ways of expressing (14) and in some sense anticipates some of the results later given in Zund and Moore (1987a).

Hence, since Hotine's conjecture is false, the system $\mathfrak{H}_3$ and its subcase $\mathfrak{H}_4$ must be rejected.

4 THE (ω, ϕ, N) COORDINATE SYSYTEM AND A CONJECTURE

In the previous section we indicated why the Hotine hierarchy $\mathfrak{H}$ contains only a single valid coordinate system $\mathfrak{H}_0$, i.e. the (ω, ϕ, N) system. We now address two issues about this system: Hotine's derivation of it, and the question relative to the generality of this system. Both of these are important in that the first reveals the inadequacy of his methodology, while the second concerns the delicate question of the generality/utility of the $\mathfrak{H}_0$ system. Finally, we conclude with an open conjecture about the $\mathfrak{H}_0$ system and its rôle in the Marussi-Hotine theory.

Hotine's construction of $\mathfrak{H}_0$ in Chapter 12 of his treatise is one of the most difficult discussions in this work. The difficulty is not that of Chapters 15 and 16 where the exposition is maddeningly sketchy, but rather that of an embarrassment of riches, where now in the possession of a coordinate system in which to express his ideas, he does everything all at once! The result is a dazzling and virtuoso performance which, given Hotine's computational ability and agility, must be experienced to be appreciated. Unfortunately, the result is a melange of equations: some relevant to the construction at hand, with others quite irrelevant and of dubious applicability to differential geodesy. To understand his procedure, one must carefully delete the unnecessary material, or to borrow a famous comment of Dirac 'eliminate the dead-wood' from the construction. This was done by the author in Zund (1993), and it becomes clear that Hotine trivialized the construction by an ingenious procedure which avoided making *full use* of the *curvature parameters*:

$$(*) \qquad k_1, k_2, t_1, \gamma_1, \gamma_2;$$

which, in fact, *he introduced* in his derivation! His presentation is flawed by *two* crucial omissions. First, having observed as early as in Hotine (1957a) that the geometry of the geopotential field, i.e. its family of equipotential surfaces $\mathbf{\Sigma}$ and plumblines $\mathbf{\Gamma}$, can be described by the parameters $(*)$, he failed to note that this description was not complete. Indeed, instead of the *five* scalars exhibited in $(*)$, in general there are *nine* scalars, which we call *leg coefficients*:

$$(**) \qquad k_1, k_2, t_1, t_2, \gamma_1, \gamma_2, \sigma_1, \sigma_2, \varepsilon_3.$$

[3] I am indebted to Prof. M. Caputo for bringing this to my attention, and kindly furnishing me with a copy of this thesis in 1993.

His work in Chapters 7 and 8 accounted for the relationship $t_2 = -t_1$ and the geodesic curvatures σ_1, σ_2; but not the scalar ε_3 (which was an important ingredient in resolving the Hotine Conjecture). Second, he nowhere exhibited the system of differential equations which the leg coefficients (**) must satisfy in order that the family $\mathbf{\Sigma}$, or more precisely, a single surface **S** of this family, be locally isometrically imbedded in $\mathbf{E}_3$ (recall the Marussi Condition (ii) of Section 2). We call these equations the *Hotine-Marussi equations* $(\mathcal{HM})$ since *some* of them do occur in their work. However, the full set $(\mathcal{HM})$ is never cited in general form, and could not be done since one of the key va.. bles ε_3 was unavailable to him. It turns out that for the $\mathfrak{H}_0$ construction, these equations reduce to a minimal set of eleven equations. Hotine's procedure was to follow an approach which formally avoided the full set $(\mathcal{HM})$, but which — almost miraculously insured that these equations were identically satisfied! The difficulty was that it did not pin down, i.e. completely specify, the values of the leg coefficients (**). Moreover, as he himself had argued, these scalars — at least those given in (*) are the basic quantities which occur in the description of the geometry of the geopotential field! In other words, the exposition he gave in Chapter 12 is incomplete in that the reader is given the impression that there is some freedom left in the specification of the parameters (*), whereas in reality this arbitrariness is limited by set $(\mathcal{HM})$. A concrete example of this ambiguity was indicated in Zund (1990b) in connection with the degeneracy the 'ω-degeneracy' — which occurs in the definition of the longitudinal coordinate ω. Hence, we can conclude that, by virtue of the material given in Zund (1993), the $\mathfrak{H}_0$ system is a valid local coordinate system in $\mathbf{E}_3$. However, Hotine's derivation is not adequate to conclusively establish this validity.

The above critique of Hotine's discussion of the (ω, ϕ, N) system leads to the following question: To what extent did Hotine properly understand what was involved in the construction of the $\mathfrak{H}_0$-system? Our answer is that he did not fully understand the structural aspects of the problems, and was likely misled by the apparent familiarity of the issue with the usual practice of employing the longitude ω and latitude ϕ as the first two coordinates in the $\mathfrak{H}_0$-system. Hence, regarding the construction as working with *known coordinates*, he did not ask the more general question of how to introduce coordinates in a less familiar situation. Indeed, Hotine's trivialization worked precisely because he knew the geometric significance of the $u^\alpha = (\omega, \phi)$ and could write down 'geometrical equations', (see Section 3 of Zund (1993)), relating them to ordinary Cartesian coordinates (x, y, z) in $\mathbf{E}_3$. This fortuitous — and unique! — circumstance permitted him to obtain equations which ultimately identically satisfied the set $(\mathcal{HM})$. Moreover, this observation also explains why his attempted derivations of the $\mathfrak{H}_1$ and $\mathfrak{H}_2$ were inadequate. In these systems he knew that one could not choose $u^\alpha = (\omega, \phi)$, but misled by his 'success' with the $\mathfrak{H}_0$-system he did not realize that unconsciously he had overlooked the essential structural features of the construction, viz. the set $(\mathcal{HM})$. Moreover, Marussi's coolness, or lack of enthusiasm, for the material in Part II cf Hotine's treatise probably suggests that he was less than convinced by Hotine methodology. Otherwise, one would have expected him to try to complete his friend's work, something which conspicuously he did not attempt to

do.

The question of the generality of the $\mathfrak{H}_0$-system is much more elusive, and difficult to settle. Clearly, both Marussi and Hotine believed — or at least fondly hoped — that it was a very general system. However, apart from stating it (recall the Hotine quotations cited in Section 3), neither of them produced any substantive argument purporting to prove its generality. Their probable justification was merely to invite the skeptical critic to examine the complicated character of the line element ds^2 of the $\mathfrak{H}_0$-system (see equation (12.069), page 77, of Hotine (1969)) where the components g_{rs} of the metric tensor were exhibited. It is indeed a complicated line element, however, recently Wilkes and Zund (1994) have produced evidence which strongly suggests that its 'generality' is illusory. Based on the notion of *orthogonality specializations*, which are a set of three specializations which ultimately reduce the general $\mathfrak{H}_0$ line element ds^2 to diagonal form we have proven the remarkable and surprising result that the only permissible specialization of $\mathfrak{H}_0$ is that one can take the geodesic torsion t_1 to be zero on the N-surface. The other specializations are invalid — indeed, they collapse the (ω, ϕ, N) system to ordinary spherical polar coordinates (ω, ϕ, r) in $\mathbf{E}_3$! The derivation is lengthy and tedious since of necessity it entails a careful analysis of the eleven equations of $(\mathcal{HM})$. Thus, if one took the view that a situation was only as general as the number of useful specializations it possesses, then the $\mathfrak{H}_0$-system cannot be regarded as being very general. The Wilkes-Zund argument, of course, does not prove the lack of generality of the $\mathfrak{H}_0$-system, however, this is the obvious implication since diagonal line elements are quite reasonable things to have in differential geodesy, e.g. the orthogonal curvilinear coordinate systems possess such line elements. On the other hand, the reduction of the (ω, ϕ, N)-system to an (ω, ϕ, r)-system when one imposes the conditions:

$$t_1 = 0 \text{ and } \gamma_1 = \gamma_2 = 0 \tag{15}$$

on the leg coefficients in $(*)$ does inescapably demonstrate the delicacy on the $\mathfrak{H}_0$-system.

As a preliminary to the conclusion of our discussion of the $\mathfrak{H}_0$-system, we now add two additional conditions to the eight Marussi conditions stated in Section 2. These two conditions seem especially relevant to questions of handling the set $(\mathcal{HM})$ which now appear as the central feature of the **MH**-theory. These conditions are:

(ix) the equipotential surfaces are general convex surfaces;

(x) assuming appropriate smoothness hypotheses on the equipotential surfaces and the geometric quantities associated with them.

In effect, the addition of these conditions provides a setting in which one can ask and have some expectation of obtaining sensible answers about solutions of the set $(\mathcal{HM})$.

Condition **(ix)** is highly non-trivial but essentially corresponds to what one expects equipotential surfaces to look like. For our purposes, by a convex surface we mean a surface which

1) can be regarded as a 2-dimensional oriented Riemannian manifold,

2) is simply connected, and

3) has strictly positive Gauss curvature K.

Condition **(x)** is required to insure that the quantities on which derivatives have been taken are well-defined, i.e. everything that should be continuously differentiable of a certain order is continuously differentiable. The order of differentiability varies from situation to situation, hence, the adjective 'smooth' is employed in a general all encompassing sense as including whatever order of differentiability is required in a given situation. This condition is crucial in establishing existence and uniqueness theorems for both equipotential surfaces and the leg coefficients, viz. (*) or (**) appearing as unknown functions in the set $(\mathcal{HM})$.

The preceding discussion of the $\mathfrak{H}_0$-system now leads us to make the following:

Conjecture

Up to a smooth change of coordinates/parameters, the $\mathfrak{H}_0$-system is the only class of local coordinate systems permitted in the **MH**-*theory.*

Before discussing in detail the significance of this conjecture, we must clearly understand its content. The first part means that by means of a suitable change of curvilinear coordinates, or Gaussian parameters, any curvilinear coordinate system x^r appearing in the **MH**-theory can be reduced to the $\mathfrak{H}_0$-system. Hence, there is not a single (ω, ϕ, N) coordinate system, but rather a class of such systems which are equivalent to it in the sense that by an appropriate change of variable they can be reduced to it.

We do not have a proof of this conjecture or realistically see how a proof could be done. In effect, it is a 'non-existence' statement and such things are notoriously difficult to prove in mathematics. Of course, the conjecture would be disproven if one could produce a coordinate system which satisfied the tenets of the **MH**-theory, viz. the Marussi Ansatz $\mathfrak{A}$, and a reasonable number of the Marussi Conditions **(i)**-**(x)**. By the latter we mean that the offered counterexample should not violate the *spirit* of these conditions, i.e. be pathological.

One can advance two general observations tending to favor the conjecture. First, in the twenty-five years since the publication of Hotine's treatise, no one has offered anything better than the $\mathfrak{H}_0$-system within the context of the **MH**-theory. Indeed, if one goes back to Marussi's original proposal the date can be pushed back forty-seven years. The second reason is that having taken $\mathfrak{A}_1$, then $\mathfrak{A}_2$ seems quite reasonable. Moreover, since one expects **S**: N = constant to be a curved surface, then the parameters $u^\alpha = (U, V)$ on it should be curvilinear, viz. angles!, and a choice of angles cannot differ much from (ω, ϕ) if they are correctly defined. For example, there is no essential difference between latitude and colatitude since one can easily convert one into the other by a simple equation.

5 GENERALIZED MARUSSI-HOTINE THEORY AND CONCLUSIONS

The results of Sections 3 and 4 suggest that the scope of the **MH**-theory is limited, and, if indeed our conjecture is valid, then the entire theory essentially is that of a single coordinate system! Such a conclusion certainly does not justify the elaborate formalism set forth by Hotine in his treatise. However, as we now will suggest, even though the **MH**-theory is limited, the formalism admits a more general interpretation which can be regarded as the basis of a new approach to differential geodesy which we call the *generalized Marussi-Hotine*, (**gMH**), *theory*. In some respects the **gMH**-theory is radical in the sense that it involves a critical rethinking of how coordinates are introduced into differential geodesy, and although we believe it offers a bold new approach to the subject, it would be quite unjustified to claim that either Marussi or Hotine would necessarily be receptive to our efforts. Nevertheless, the appellation 'generalized **MH**-theory' is appropriate since it is a generalization of their theory, and upon being given a geopotential function N and imposing ($\mathfrak{A}$), the generalized **MH**-theory does formally reduce to the **MH**-theory. There is, however, a fundamental difference in how local coordinates are viewed in the two theories. In order to explain this feature we must now consider the various role of coordinates in theoretical geodesy — both in classical theory and differential geodesy.

Following Zund (1992), we may distinguish between *three* different types of coordinates:

determinative, descriptive, and permissible coordinates.

In the first two cases, coordinates are the primary variables of the theory and geometric paramters (such as those displayed in (*) and (**) of Section 4) play a secondary rôle. *Determinative coordinates* are those of classical theoretical geodesy and are so named since they are the coordinates employed in the determination of the geopotential function N from an appropriate Laplace-like equation. Usually they are chosen for their efficacy in solving the basic equations, and although they may be related to a special symmetry exhibited in the geodetic situation, they need not be convenient for the purposes differential geodesy. The coordinates of the **MH**-theory are purely *descriptive* and presume that a geopotential function N is known. Then by using $\mathfrak{A}$ one seeks to describe the geometric properties of the given geopotential field. This requires satisfying the set ($\mathcal{HM}$) and, in effect, involves a considerable degree of guesswork since the derivatives appearing in the set ($\mathcal{HM}$) are directional derivatives, i.e. the projections along the directions of the Hotine vectorial 3-leg $\{\boldsymbol{\lambda}, \boldsymbol{\mu}, \boldsymbol{\nu}\}$. These 3-leg vectors are respectively the tangent vectors to the equipotential surface **S**, and the normal to this surface (which is, hence, a tangent vector of the plumblines of **S**). The coordinates occur as variables in the components of these vectors, and while these x^r are known the values of the components of the 3-leg vectors must be deduced. By virtue of a theorem in the Ricci congruence calculus, the nine components of the three 3-leg vectors are equivalent to the nine leg-coefficients exhibited in (**). Thus, for

example, in the case of the $\mathfrak{H}_0$-system, the **MH**-theory seeks to *fit a geometry* to the families $\mathbf{\Sigma}$ and $\mathbf{\Gamma}$ in $\mathbf{E}_3$. The example of Wilkes and Zund, cited in Section 4 shows that *some* choices of the leg coefficients are incompatible with the structure of the $\mathfrak{H}_0$-system. Indeed, if our conjecture is valid it is very likely that possible values of the leg coefficients $(**)$ are severely limited, since the entire theory would be rigidly tied to those particular geodetic situations which are readily adaptable to the $\mathfrak{H}_0$-system.

Our proposal to remedy this restrictiveness is to approach the situation in a totally different manner, and regard the primary variables as being the set of leg coefficients $(**)$ with the geopotential being an *unknown* function. We would then seek to solve the set $(\mathcal{HM})$ subject to the general rules of the Marussi Conditions **(i)**-**(x)** and some geometric assumptions on the leg coefficients $(**)$. The latter serve as Cauchy data, i.e. 'boundary conditions', for the solution of the Cauchy problem of solving the set $(\mathcal{HM})$. For example, one might ask for a solution of $(\mathcal{HM})$ such that

$$K > 0 \Longleftrightarrow k_1 k_2 - t_1^2 > 0,$$

with

$$\chi \neq 0 \Longleftrightarrow \gamma_1, \gamma_2 \text{ not both zero;}$$

i.e. for a convex **S** having curved plumblines. Typically, either one will be able to solve the set $(\mathcal{HM})$ (possibly with additional assumptions); or the assumptions will be incompatible with the set $(\mathcal{HM})$. In the former case, one has constructed a geopotential field (i.e. a geopotential function N) having prescribed geometric properties; while in the latter case, one would see that the chosen assumptions are inadequate/inconsistent with the set $(\mathcal{HM})$ — viz. there the desired solution is not possible. Given a solution, one then derives a coordinate system x^r associated with the obtained leg coefficients/components of the 3-leg vectors. Such coordinates are said to be *permissible*, since they are the coordinates permitted by the Cauchy data and solution of the Cauchy problem. These coordinates should be physically useful, since *by construction* they possess certain geodetic/geometric properties.

One might regard such an approach as overly difficult and unrealistic, except for that fact that it is essentially the procedure employed by Kerr in his solution of Einstein's equations. These equations are much more difficult than the set $(\mathcal{HM})$, and, hence, one may reasonably expect with some effort and ingenuity that there is a good chance of success of solving them. Indeed, there is a close analogy between the set $(\mathcal{HM})$ and the corresponding formulation of the Einstein equations. The latter has been given in several forms, however, the most useful is due to Newman and Penrose (1962) and known as the *Newman-Penrose*, $(\mathcal{NP})$, *equations.* It involves twelve complex quantities which are known as *spin-coefficients*, which are analogous to our leg-coefficients $(**)$. The difference is that — as Hotine observed — the leg-coefficients have an immediate geometric interpretation, whereas the interpretation of the spin-coefficients is less apparent since it involves a four-dimensional Riemannian geometry. Actually, it is ironic in that Hotine recognized the significance of the curvature parameters $(*)$, but apparently not that they were part of a more general formalism, i.e. the Ricci congruence calculus, or what we have called the *leg calculus.* This formalism dates back into the years 1895-1901 and the original work of G.

Ricci [1853-1925] and his pupil T. Levi-Civita [1873-1941] on the tensor calculus. Unfortunately, in their attempt to sketch the framework of n-dimensional Riemannian geometry, they did not develop the $n = 2$ and 3-dimensional theory to the extent that it became an alternative way of treating Gaussian differential geometry. Although some of this was included in Cartan's theory of exterior differential forms, its applicability to the classical differential geometry of curves and surfaces is relatively new and unexplored. The Newman-Penrose formalism offers serveral advantages which we can expect will be carried over to the $(\mathcal{HM})$ equations. First, it permits the solution of the set subject to certain geometric/physical assumptions; and second, it specifies a procedure — a definite order — in which the equations are integrated. As this procedure is followed, one successively restricts the freedom of the coordinate system which is derived from the values of the spin-coefficients and the components of the leg vectors.

In conclusion, I feel obliged to offer a few personal comments about differential geodesy and the work of Marussi and Hotine, in particular. I came to theoretical geodesy some ten years ago after nearly twenty years of active research in geometry and mathematical physics (my academic training was in mathematics). While geodesy is obviously a rather more mundane discipline than either of these subjects one certainly hopes it is more 'down to Earth' than either Kummer's quartic surface or the theory of gravitational radiation and black holes! In studying the contributions of Marussi and Hotine, I have found no dimunition of depth and ingenuity in their ideas than those I previously experienced in the writings of Einstein, Dirac, and Weyl. It has been my privilege to learn from Marussi and Hotine, and I have found it to be the most challenging and satisfying period of my academic research. Not only have I found differential geodesy to be exciting and intriguing, but the geodetic community at large has been unstinting in its encouragement and generous support of my efforts. I hope that my forthcoming monograph, Zund (1994/95), will be regarded not only as a tribute to Marussi and Hotine, but also a partial repayment of my debt to all geodesists for their many kindnesses (both personal and professional) to me.

The vision of a geometric formulation of geodesy which Marussi and Hotine saw is undiminished by their passing from us. I believe that the words of the great French physiologist and anatomist Claude Bernard [1813-1878] well describe them:

> "Great men may be compared to torches shining at long intervals, to guide the advance of science. They light up their time, either by discovering unexpected and fertile phenomena which open up new paths and reveal unknown horizons, or by generalizing acquired scientific facts and disclosing truths which their predecessors had not perceived."

REFERENCES

G. Bomford (1949)
Report — Section V — Géoïde, *Bulletin Géodésique* - Comptes rendus d'ensemble de la VIII^e Assemblée Générale (Oslo, août 1948), 75-77.

A. Cayley (1895)
Coordinates versus quaternions, *Proceedings of the Royal Society of Edinburgh* 20 , 271-275 = paper number 962 of *Collected mathematical papers* XIII, Cambridge: Cambridge University Press (187), 541-544.

A.R. Forsyth (1920)
Lectures on the differential geometry of curves, and surfaces, Cambridge: Cambridge University Press [see Chapter XI].

M. Hotine (1957a)
Metrical properties of the earth's gravitational field, report to the I.A.G. Toronto Assembly = 33-64 of Hotine (1991).

M. Hotine (1957b)
Geodetic coordinate systems, report to the I.A.G. Toronto Assembly = 65-89 of Hotine (1991).

M. Hotine (1965)
Trends in mathematical geodesy, *Bollettino di Geodesia e Scienze Affini*, anno XXIV, 607-622 = 5-21 of Hotine (1991).

M. Hotine (1966a)
Triply orthogonal coordinate systems, *Bulletin Géodésique* 81, 195-222.

M. Hotine (1966b)
Note by the writer of the paper on 'Triply orthogonal coordinate systems', *Bulletin Géodésique* 81, 223-224.

M. Hotine (1969)
Mathematical geodesy, Washington, D.C.: U.S. Department of Commerce.

M. Hotine (1991)
Differential geodesy, edited by J. Zund, Berlin: Springer-Verlag.

R.P. Kerr (1963)
Gravitational field of a spinning mass as an example of algebraically special metrics, *Physical Review Letters* 11, 237-238.

R.P. Kerr (1965)
Gravitational collapse and rotation, in *Quasi-stellar sources and gravitational collapse*, edited by I. Robinson, A. Schild and E.L. Schücking, Chicago: University of Chicago Press, 99-102.

A. Marussi (1947)
Sulla struttura locale del geoide, e sui mezzi geometrici e mecanici atti a determinarla, *Annali Triestini*, 17 (Ser. IV), 1-31.

A. Marussi (1949)
Fondements de géométrie différentielle absolue du champ potentiel terrestre, *Bulletin Géodésique* 14, 411-439.

A. Marussi (1951)
Fondamenti di geodesia intrinseca, *Pubblicazioni della Commissione Geodetica Italiana*, (Ser. III) 7, 1-47 = 13-58 of Marussi (1985).

A. Marussi (1985)
Intrinsic geodesy, translated by W.I. Reilly, Berlin: Springer-Verlag.

A. Marussi (1988)
Intrinsic geodesy (a revised and edited version of his 1952 lectures prepared by J. Zund), Report No. 390, Department of Geodetic Science and Surveying, Columbus: The Ohio State University.

N. Milani (1960/61)
Sul significato intrinseco dell'equazione cui soddisfano le famiglie di Lamé, *Tesi di Laurea in scienze matematiche*, Università di Trieste.

E.T. Newman and R. Penrose (1962)
An approach to gravitational radiation by a method of spin coefficients, *Journal of Mathematical Physics* 3, 566-578.

J.L. Synge and A. Schild (1949)
Tensor calculus, Toronto: University of Toronto.

C.E. Weatherburn (1938)
An introduction to Riemannian geometry and the tensor calculus, Cambridge: Cambridge University Press.

J.M. Wilkes and J. Zund (1994)
Specializations of Hotine's (ω, ϕ, N) coordinate system in differential geodesy, *Bollettino di Geodesia e Scienze Affini*, anno LIII, in press.

J. Zund (1990a)
An essay on the mathematical foundations of the Marussi-Hotine approach to geodesy, *Bollettino di Geodesia e Scienze Affini*, anno XLIX, 133-179.

J. Zund (1990b)
The assertion of Hotine on the integrability conditions for his general (ω, ϕ, N) coordinate system, *Manuscripta Geodaetica* 15, 362-372.

J. Zund (1991)
The work of Antonio Marussi in theoretical geodesy, Proceedings of the geodetic day in honor of Antonio Marussi (Rome, October 9, 1989) *Atti dei Convegni Lincei* 91 Roma: Accademia Nazionale dei Lincei, 9-20.

J. Zund (1992)
Coordinates in differential geodesy, *Phillips Laboratory Research Report* PL-TR-92-2072, 1-9 [presentation at XX General Assembly of the I.A.G., Vienna, August 1991]. **ADA254951**

J. Zund (1993)
Hotine's (ω, ϕ, N) coordinate system, *Phillips Laboratory Research Report* PL-TR-93-2174, 1-23. **ADA270108**

J. Zund (1994)
Foundations of differential geodesy, Berlin: Springer-Verlag.

J. Zund (1994*)
Unsolved puzzles in differential geodesy, in preparation for *Section IV Bulletin General Theory and Methodology.*

J. Zund and W. Moore (1987)
A note on the Cayley-Darboux equation, *Tensor NS* 45, 202-205.

J. Zund and W. Moore (1987)
Hotine's conjecture in differential geodesy, *Bulletin Géodésique* 61, 209-222.